

tials which appeared in these columns a short time ago, but with some additional matter, must, from the clear explanation of the principles involved in the different methods of measurement, take a high position as an educational work, and, from the care with which details of manipulation are in many parts described, form a valuable laboratory guide.

The author begins by explaining Gauss's method of finding the horizontal intensity of the earth's magnetism. Instead of describing an "instrument-maker's" magnetometer, and showing how with this expensive luxury  $H$  may be determined, he gives simple, clear, and full directions for constructing, with such common materials as are to be found in any laboratory, all that is necessary for making this determination with great accuracy.

A description of the tangent galvanometer in some of its forms and an explanation of some of the units naturally follow. Here, by treating each unit separately with many illustrations depending on the aspect from which they are viewed, the author has succeeded in giving them a reality which students often find it difficult to believe they possess.

The next two chapters are devoted to a description of the construction and graduation of Sir W. Thomson's "Graded Galvanometers." These instruments possess so great a range, and are, when used carefully in the laboratory, so accurate and convenient, though rather delicate for an engine-room, that an exact description from headquarters of their construction, of the precautions which must be observed in their use, and of the means of graduating them is especially valuable.

The various methods employed in measuring any resistance from that of a thick copper rod to that of a piece of gutta-percha are given, and in many cases explained by numerical examples.

The methods by which the energy due to direct or to alternating currents may be measured is explained—in the latter case on the assumption that the current strength varies harmonically with the time.

The chapter on the measurement of intense magnetic fields is especially interesting, for the methods given, depending on the use of suspended bits of wire attached by threads to pendulum weights, or equally simple and easily contrived devices, show how the experimenter may in many cases be independent of the elaborate work of the instrument-maker.

C. V. B.

*Field and Garden Crops of the North-Western Provinces and Oudh.* By J. F. Duthie, B.A. F.L.S., Superintendent of the Saharanpur Botanical Gardens, and J. B. Fuller, Director of Agriculture, Central Provinces. Part 2. With Illustrations.

As a work of reference it will be very valuable, for it contains well-arranged details of some of the more important crops under cultivation, and the information is well and systematically arranged. Care has been taken in each case to secure a complete but still a concise statement, which is sufficient to guide the cultivator in all the specialities of management necessary to secure successful results. A good drawing illustrates each crop treated of, and its several cultivated varieties, and with these we have carefully-prepared descriptions of each plant in succession, and its general history. The districts within which the cultivation can be successfully extended are also set forth with great clearness and precision. For accuracy of details, in a very accessible form, this work leaves little to be desired.

*A Treatise on Higher Trigonometry.* By the Rev. J. B. Lock. (Macmillan, 1884.)

THIS is the promised complement to the same writer's "Treatise on Elementary Trigonometry," which we noticed very favourably in these pages at the time of its appearance (vol. xxvi. p. 124). It is concerned principally

with series, the errors which arise in practical work, and the use of subsidiary angles in numerical calculations.

A short chapter on the use of imaginaries is justified by the position this subject holds in the London University Examinations, and no apology is needed for the space assigned to an account of, and a collection of exercises upon, the hyperbolic sine and cosine. We have read the text carefully, and though almost of necessity there are numerous typographical mistakes, only one or two (for  $2a \cos 2\theta$ , p. 127, line 3, read  $a \cos 2\theta$ ) will inconvenience a student. In addition to the numerous examples in the text, there are fourteen specimen papers from Cambridge and other examinations.

The only article to which we take exception is § 9, the proof of which may be, if we mistake not, considerably simplified. The book can be confidently recommended to the use of advanced pupils in our schools, and will meet the wants of most students in our Universities.

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

### Teaching Animals to Converse

You did me the honour some weeks ago (January 3, p. 216) to insert a letter of mine, containing suggestions as to a method of studying the psychology of animals, and a short account of a beginning I had myself made in that direction.

This letter has elicited various replies and suggestions which you will perhaps allow me to answer, and I may also take the opportunity of stating the progress which my dog "Van" has made, although, owing greatly no doubt to my frequent absences from home, and the little time I can devote to him, this has not been so rapid as I doubt not would otherwise have been the case. Perhaps I may just repeat that the essence of my idea was to have various words, such as "food," "bone," "water," "out," &c., printed on pieces of cardboard, and after some preliminary training, to give the dog anything for which he asked by bringing a card.

I use pieces of cardboard about 10 inches long and 3 inches high, placing a number of them on the floor side by side, so that the dog has several cards to select from, each bearing a different word.

One correspondent has suggested that it would be better to use variously coloured cards. This might no doubt render the first steps rather more easy, but, on the other hand, any temporary advantage gained would be at the expense of subsequent difficulty, since the pupil would very likely begin by associating the object with the colour rather than with the letters; he would, therefore, as is too often the case with our own children, have the unnecessary labour of unlearning some of his first lessons. At the same time the experiment would have an interest as a test of the condition of the colour-sense in dogs. Another suggestion has been that, instead of words, pictorial representations should be placed on the cards. This, however, could only be done with material objects, such as "food," "bone," "water," &c., and would not be applicable to such words as "out," "pet me," &c.; nor even as regards the former class do I see that it would present any substantial advantage.

Again, it has been suggested that "Van" is led by scent rather than by sight. He has no doubt an excellent nose, but in this case he is certainly guided by the eye. The cards are all handled by us, and must emit very nearly the same odour. I do not, however, rely on this, but have in use a number of cards bearing the same word. When, for instance, he has brought a card with "food" on it, we do not put down the same identical card, but another with the same word; when he has brought that, a third is put down, and so on. For a single meal, therefore, eight or ten cards will have been used, and it seems clear, therefore, that in selecting them "Van" must be guided by the letters.

When I last wrote I had satisfied myself that he had learnt to regard the bringing of a card as a request, and that he could distinguish a card with the word "food" on it from a plain one, while I believed that he could distinguish between a card with "food" on it, and one with "out" on it. I have no doubt that he can distinguish between different words. For instance, when he is hungry he will bring a "food" card time after time until he has had enough, and then he lies down quietly for a nap. Again, when I am going for a walk and invite him to come, he gladly responds by picking up the "out" card and running triumphantly with it before me to the front door. In the same way he knows the "bone" card quite well. As regards water (which I spell phonetically so as not to confuse him unnecessarily), I keep a card always on the floor in my dressing-room, and whenever he is thirsty he goes off there, without any suggestion from me, and brings the card with perfect gravity. At the same time he is fond of a game, and if he is playful or excited will occasionally run about with any card. If through inadvertence he brings a card for something he does not want, when the corresponding object is shown him he seizes the card, takes it back again, and fetches the right one.

No one who has seen him look along a row of cards and select the right one can, I think, doubt that in bringing a card he feels that he is making a request, and that he can not only perfectly distinguish between one word and another, but also associate the word and the object.

I do not for a moment say that "Van" thus shows more intelligence than has been recorded in the case of other dogs; that is not my point, but it does seem to me that this method of instruction opens out a means by which dogs and other animals may be enabled to communicate with us more satisfactorily than hitherto.

I am still continuing my observations, and am now considering the best mode of testing him in very simple arithmetic, but I wish I could induce others to cooperate, for I feel satisfied that the system would well repay more time and attention than I am myself able to give.

JOHN LUBBOCK

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### "The Unity of Nature"

I REGRET that the Duke of Argyll should have been led by anything that I have written to make some of the remarks which appear in this week's issue of NATURE (p. 524). If a reviewer in a signed review cannot express freely his opinion upon a book without its being suggested that he is actuated by secondary and sinister motives, I fancy that few men of common honesty would care to continue the work of reviewing. Moreover, in the present instance the imputation of animus seems to me specially unjustifiable. I had almost forgotten the correspondence in NATURE to which the Duke alludes, but on now referring to it again I can only see that, if it was provocative of animus, there was assuredly no reason for the animus to have arisen on my side (see NATURE, vol. xxiv. pp. 581 and 604; vol. xxv. pp. 6 and 29). But, to ignore so unworthy a charge, and one which I can only suppose to have been made under a sense of irritation, I must explain that the Duke is under a wrong impression when he assumes that my objection to his advocacy of Theistic belief is due to what he regards as my aversion to Theism. As I have never been in the habit of "using your columns for the purpose of inculcating personal beliefs and disbeliefs on subjects which lie outside the boundaries of physical science," I shall not do so now. But in view of the slender grounds on which the Duke has felt himself entitled to infer that I "hold that the highest aim of the human intellect is to prove the mindlessness of nature," I feel it is desirable to correct the inference. For this purpose it is not needful that I should publish my "personal beliefs and disbeliefs." It is only needful to say that my previous remarks will be found to have been directed, not against the cause of Theism, but against its champion in the Duke of Argyll. Had my sympathies been more on the side of the materialists than they happen to be, the Duke of Argyll might not have found so much reason to quarrel with my "dislike" of his advocacy.

I may now turn to the Duke's remarks on those of my criticisms which he deems legitimate. Taking first the case of rudimentary organs, I quite agree with the statement that the question whether any particular structure now dissociated from use is to be regarded as "on the stocks or on the wane" is "a question of evidence from associated facts." Therefore it was that I said in my review that no illustration could be more unfortunate than the one which was chosen by the Duke as an

example of rudimentary structures possibly on the stocks. For if the rudimentary organs which occur in the Cetacea admit of being supposed of doubtful interpretation in this matter, it is clear that in no case could the "evidence from associated facts" of structure and affinity be of any value. But in reality this evidence is nearly always so cogent that the difficulty suggested by the Duke is of a purely imaginary kind: evolutionists have no need ever to be puzzled in deciding whether a given structure is on the stocks or on the wane. Thus, for instance, let us take the cases which are adduced by the Duke himself. No evolutionist could be insane enough to imagine that the papillæ on the roof of the mouth of the giraffe are the remnants of whalebone, seeing that the whole structure and all the affinities of the animal are opposed to the inference that its ancestors were aquatic mammalia. Or, if we take the case of webbed feet, even if the dipper had begun to develop them, no evolutionist in his senses would infer that these incipient structures were remnants of structures once more fully developed, seeing that all the other structures and affinities of the bird prove that it belongs to a non-aquatic family. Cases of this kind actually occur in such birds as the grebe and the coot, where even apart from structure and affinity it is easy to see that the little piece of web must be regarded as a growing and not a dwindling organ, seeing that the birds are so strongly aquatic in their habits.

Considering next the Duke's remarks on instinct, I did not attempt to deal with the argument to which he refers, because I could not perceive that there was any argument to be dealt with. His view is a mere assumption to the effect that instincts are divinely implanted intuitions independent of experience; and to deny that experience, in successive generations, is the source of instinct is not to meet, by way of argument, the enormous mass of evidence which goes to prove that such is the case. Even within the limits of my review I should have thought there was evidence enough to have disposed of this denial.

As for the special case of the dipper, I only mentioned it in my review because the Duke lays great stress upon it in his book. No doubt better cases occur of newly-acquired instincts not yet associated with correlated structures, and in all such cases (whether good, bad, or indifferent), it is not a *non sequitur* mode of argument to say that, on the theory of the transmutation of instincts, the appropriate organs have not been developed, because, looking to the affinities of the animal, we are entitled to infer that time enough has not yet been allowed for their development. Again, I deny that it is for me, or for any other evolutionist, to prove that the ancestors of the dipper did not present those lesser modifications of structure which, according to the Duke, are now correlated with the aquatic instincts.<sup>1</sup> By "proof" he no doubt means the display of the ancestral form, and not the study of allied species. Proof of this kind is not attainable, but neither is it required. The question whether instincts are fixed intuitions or admit of being modified by accumulative experience with natural selection—i.e. whether they are or are not subject to evolution—is a question that does not require to be settled on the narrow basis of any one particular case. And if we take a broad view of all the instincts known to us, the combined weight of their testimony to the fact of transmutation is simply overwhelming.

London, April 4

GEORGE J. ROMANES

### The Remarkable Sunsets

THE remarkable *red sunsets* and after-glows, about which so much has been written of late, still continue here, but in a less intense form. A remarkable one occurred last night, and while watching it I determined to send you a brief account of my experiences in the matter. It is of little use going into descriptions of the appearances which are now well known, but the one which occurred last evening was unusually fine. It was a stormy wild evening, with black clouds all around, except in the west, where, from about 10° above the horizon to near the zenith, it was quite clear, and of a pale orange glow. A quarter of an hour after sunset three immense rays through rifts in the cloud bank sprang up almost suddenly, and took quite an intense crimson lake colour, which lasted about ten minutes.

Our brightest displays occurred in October and November last, and frequently bathed the whole landscape in a deep

<sup>1</sup> I say "according to the Duke," because, according to Mr. Darwin, "in the case of the water-ouzel the acutest observer, by examining its dead body, would never have suspected its sub-aquatic habits" ("Origin of Species," 6th ed., p. 142).